PROCEEDINGS

OF THE

LINNEAN SOCIETY

OF

NEW SOUTH WALES.

WEDNESDAY, MARCH 29TH, 1899.

The Twenty-fifty Anuual General Meeting of the Society was held in the Linnean Hall, Ithaca Road, Elizabeth Bay, on Wednesday evening, March 29th, 1899.

Professor J. T. Wilson, M.B., Ch.M., President, in the Chair.

The Minutes of the previous Annual General Meeting were read and confirmed.

The President delivered the Annual Address.

PRESIDENTIAL ADDRESS.

It is gratifying to be able to report that the past Session has been characterised by satisfactory activity and progress in the Society's customary field of work, and by important developments in its founder's plans for an extended sphere of action in the future. Though not a matter which has affected the scientific life of the Society, it is nevertheless to be regretted that the number of effective Members has remained practically stationary. Five Ordinary Members were elected into the Society, one of

whom subsequently retired, four Members have resigned, and death has deprived the Society of one of the Members resident in Tasmania.

Charles Edward Beddome died at Hobart on September 1st, 1898, aged 62 years. He joined the Indian Navy as a lad, and had attained the rank of lieutenant when this branch of the service was abolished. Mr. Beddome subsequently emigrated to Queensland, and for some time filled the position of Police Magistrate at Thursday Island, and elsewhere. Still later, he retired from the Government service, and turned his attention for some years to pastoral pursuits in the Port Curtis district, where he became owner of a cattle station. Finally he removed to Tasmania, where he spent the remainder of his life.

Mr. Beddome, like his brother, Colonel R. H. Beddome, well known for his researches on the Land Mollusca of India, was an ardent conchologist. In Tasmania he dredged and collected assiduously. The importance of his own collection was enhanced by his acquisition of one formed by Mr. W. Legrand containing the series studied by the late Rev. J. E. Tenison-Woods. His papers, which are not numerous, are to be found either in the Papers and Proceedings of the Royal Society of Tasmania or in the Proceedings of this Society. Mr. Beddome was elected a member of the Society in October, 1880.

The Proceedings for 1898 form a volume of 838 pages, illustrated with thirty-three plates, and comprising forty papers contributed during the Session. These may be classified as follows: Botanical, 16; ethnological, 2; palæontological, 1; embryological, 1; zoological, 20. Three Parts of the Proceedings, containing the majority of these papers, were published and distributed last year, in addition to two Parts of the Proceedings for 1897, which remained over from the previous year. The sheets of the concluding Part are printed off, and as soon as the lithographer has finished his share of the work the Part will be issued.

By the wreck of the s.s. China on her homeward voyage in March last, the Society had the misfortune to lose a package containing despatches for thirty-four Societies or Institutions in Great Britain,

the United States, and Canada. Fortunately a number of Parts of the same issue had been sent by post, so that the package forwarded by the China was smaller than usual. A duplicate set was afterwards despatched to replace the publications thus lost. The package was insured, but the amount of the insurance is but a fraction of what it would cost to republish the Part, upon the surplus stock of which a rather serious inroad has been made.

Reference has already been made to the practically stationary condition of the Members' Roll. At a Special General Meeting to be arranged for at an early date, you will be asked to consider a recommendation from the Council that the operation of Rule vi., in so far as it relates to entrance fees, be suspended during the current year. With the same amount of capital invested, the Hon. Treasurer has had to struggle for several years past with a diminution in the annual income of about £200 per annum, due to the fall in the rate of interest on sound investments since the recent commercial crisis.

The annual subscription for original members (*i.e.*, those who joined in 1874) was one guinea, without entrance fee; from 1875-84 one guinea, with an entrance fee of one guinea. In 1885 this was altered to two guineas per annum, without entrance fee; and for Associate Members one guinea, without entrance fee. From 1893 to the present time the rates have been one guinea for all Members, with an entrance fee of two guineas for new Members; for Associate Members, one guinea, with an entrance fee of one guinea. In other words, under the present régime a Member pays for the first two years the same amount and no more than he would pay in the same period with an annual subscription of two guineas, without entrance fee, while thereafter he would pay only half. This alteration was made after the Society came into possession of the gifts and bequests of the late Sir William Macleay, but before the commercial crisis; and was adopted without hesitation, though at the time it was evident that it would involve a slight diminution in the annual income. It was meant to be an expression of the feeling that it would harmonise with Sir William's liberality to the Society if financial

considerations had as little as possible to do with the exclusion of otherwise desirable candidates for membership. The amount annually exacted from effective members of the Society compares more than favourably with that due by members of other Australasian Societies of the same standing. The volumes of the Proceedings too are larger, and the printing and illustrations proportionally more costly than was the case in earlier years.

A year ago I was able to announce the appointment of Mr. R. Greig Smith, M.Sc., Lecturer in Agricultural Chemistry at the Durham College of Science, Newcastle-upon-Tyne, to be the first Macleay Bacteriologist. Mr. Smith arrived from England in September last and at once entered upon his duties. The first matter for consideration was the transformation of a large empty room into a laboratory as well fitted up and equipped for research as the resources at the disposal of the Council would permit. By November sufficient progress had been made to enable the Council to consider the plans and a scheme of expenditure submitted by the advisory sub-committee in conjunction with Mr. Smith, involving an outlay of about £660. A tender for tiling the floor was accepted, and a little later a second for the supply and fixing of the necessary fittings. I regret to say that through a disastrous accident to the kiln in which the tiles were being burnt, the first of these has not yet been carried out, but we have a promise that any further delay will not exceed three weeks. The second contract was finished within the time specified in February last, at a cost of £164 11s. This provides for the whole of the fittings and the fixing thereof, including cupboards, benches, tables, shelves, photographic room, the laying on of gas and water, hoods for carrying off heated air, and venetian shutters or blinds to the windows. You will have the opportunity this evening of seeing for yourselves what has so far been accomplished in this direction.

As regards equipment, progress may be reported as follows :— Mr. Smith was authorised before his departure from London to select and bring with him apparatus and chemicals either of a special character or as necessary for an interim equipment, to the extent of £70. Since his arrival two orders have been sent to

Europe for optical, bacteriological or other apparatus, and chemicals to cost about £200. The goods thus ordered should ere now have been despatched to their destination. In addition certain apparatus and supplies of chemicals have been obtained locally at a cost of £16.

When the arrangements still in contemplation or now in course of execution are completed, it will be conceded that the Society may be congratulated on the improvement effected in the Linnean Hall, and on its acquisition of a laboratory sufficiently well equipped to allow of bacteriological researches being systematically carried out under its auspices, and thus of adding to its importance and enlarging its sphere of influence. Very careful consideration throughout has been given to the subject, and while luxurious or extravagant expenditure has been avoided, no effort has been spared to make the available resources go as far as possible in providing a laboratory primarily of a utilitarian character. I need hardly say that throughout Mr. Greig Smith has heartily co-operated with the advisory sub-committee and the Council in carrying out the improvements.

With the arrival of the balance of the equipment, and the completion of the tiling of the floor, the last of the hampering restrictions to some extent now operating will disappear, and the Bacteriologist will then be in a position to settle down to steady work. As this is the first of our annual gatherings at which Mr. Smith has been present, I take the opportunity on behalf of the Society of offering him a hearty welcome, and of wishing him a very successful career in his new sphere of work.

In its capacity as trustee, the Society may, on this occasion, be congratulated that Sir William Macleay's intentions and directions are now on the point of realisation. It is not necessary to recapitulate the circumstances under which the trust unexpectedly devolved upon this Society. As far as the Society is concerned they may be allowed to drop out of mind. The bequest was an alternative one, but the Society was not concerned in taking the initiative or an active part in bringing about the final result. It is true that fully seven years have now elapsed since probate

of Sir William's will was granted ; but when allowance is made for the vicissitudes which for some time attended the carrying out of the terms of the bequest, and especially for the very serious commercial depression which meantime overtook the community, it may fairly be said that throughout the Council has faithfully endeavoured to administer the trust, and that its policy of proceeding slowly and deliberately has been a commendable one.

As far as the Society is concerned, effect has now been given to the trust. It now rests with the Macleay Bacteriologist and his successors to justify Sir William Macleay's conviction that it was a desirable thing for the scientific welfare of Australia that the status of Bacteriology should be raised; and that one effective way of accomplishing this was by the appointment of a Bacteriologist untrammelled by official or routine duties, and free to engage in research to the full extent of his ability and enthusiasm.

When, a year ago, I had the honour of addressing you from this chair, I chose for the subject of my remarks during a portion of my address, the somewhat threadbare question of how far mechanical, *i.e.*, physico-chemical, theories are capable of being utilised in the explanation of the phenomena of living activity.

I ventured to state the conviction that, in so far as a strictly scientific or natural-historical representation of these phenomena is the object aimed at, this can only be given in terms of physical cause, or mechanism.

By "strictly scientific or natural-historical explanation," I understand one which is susceptible of verification and of advancement by the objective and experimental methods of scientific procedure, which, as it appears to me, must necessarily operate upon the plane of physical causality. For experimental science, the world-order is conceived as a purely causal nexus.

It was pointed out at the same time that the validity of any such method of explanation was not absolute, but was relative to a particular aspect of reality; and that its adoption as the characteristic working conception of scientific procedure, does not

preclude the necessity for an interpretation of the phenomena of life from the point of view of a philosophically more adequate synthetic principle than the elementary and abstract category of causality.

And there can be no doubt that this limitation of the conception of cause as a principle of synthetic interpretation becomes specially evident in the effort to apply it to the phenomena of life. In other words, the conception of mechanism fails to satisfy the demand of the intelligence for an explanation of the co-ordinate differentiation of living parts, and the co-ordinate and purposive adaptation to ends, which seem everywhere to be such characteristic features of organisation.

It was further insisted that the notion generated by the consideration of these features is one which is undeniably and radically distinct from that of mechanical causation, involving as it does the idea of determination by "consequent" rather than by "antecedent," which is the differential characteristic in all operations of mechanism.

Reason was also given for the conviction that the teleological notion of purpose—*i.e.*, of determination by end, or consequent—may not be "put aside as a mere preliminary illusion of the intelligence—as a fiction that we accustom ourselves to suppose," but on the contrary that it embodies for us a true and genuine aspect of reality.

The ultimate interpretation of organism in terms of purpose brings us, indeed, closer to reality than any merely mechanical one can ever do. For the conception of purpose does not negate mechanism; it includes, while it re-interprets it. The idea of determination by ends involves that of the means whereby the ends are realised. And in living organisms these means are necessarily chemical and physical, *i.e.*, in the broad sense mechanical. From this point of view, physical and chemical events themselves can no longer be regarded merely as causally determined links in an endless chain of transformations of energy. Such a view of them is partial, abstract, and schematic, and is thus in the strictest sense *unreal*.

The necessity for a recognition of the general principle of determination by ends as a synthetic and unifying principle of interpretation, has inspired at various epochs the advocates of what is called "vitalism" in biology. The older vitalism, of which it has been well said that it was merely "mechanism misunderstood," like the old theological "design argument," has served to bring "teleology" into disrepute during the greater part of the century. To disparage this much abused principle has been a shibboleth of not a little of the later biological literature of the century. It is to modern philosophical criticism that we are indebted for what I believe to be a clearer insight into the relative validity of the two principles of cause and purpose respectively, as applied to the interpretation of phenomena. Through it we may learn that the recognition of purpose in the interpretation of nature does not necessarily involve the intrusion of a new extraneous, super-physical form of "vital" energy. This would be "mechanism misunderstood." But through it we also learn to discard the widely prevalent view that the principle of mechanical causation, which forms the governing conception of physics end chemistry as scientific disciplines, is therefore to be regarded as the sole and only synthetic principle by which we can connect phenomena in the unity of a single system.

Having devoted a considerable portion of my former address to the attempt to set forth the position just outlined, I should have thought it unnecessary to return to it on the present occasion but for the circumstance that in the interval there has appeared in the issue of the "Nineteenth Century" for September, 1898, a contribution towards the discussion of this very question of "vitalism" versus "mechanism." A consideration of this may, on the present occasion, be deemed neither out of place nor wholly unprofitable.

The article in question is from the pen of my friend Dr. J. S. Haldane, Lecturer on Physiology in the University of Oxford, whose previous utterances on the same subject, together with his very high reputation as an experimental physiologist, entitle him

to speak with some authority on behalf of that school of biologists to which the term "neo-vitalist" has been applied.

I am the more anxious to take note of the interesting essay referred to on account of the fact that in my last year's criticism of the neo-vitalist position it was Dr. Haldane's exposition of that position that I mainly relied upon, quoting at some length from a published essay of a good many years ago. It was thus with a great deal of interest that I perused the re-statement of the same position in his recent article.

A brief examination of the argument of this article may serve to bring the points at issue into prominence.

After pointing out that mechanical doctrines respecting the phenomena of life became dominant during the last fifty years in coincidence "not only with great advances in physics and chemistry, but also with the appearance of plausible physical and chemical theories to explain some of the most fundamental physiological processes," the writer follows up "some of the main lines in the development of the physico-chemical movement of recent times." And he endeavours to show in the case of the instances chosen —and they might be easily added to —that theories which treat cell-growth and nutrition as mere mechanical or chemical aggregation; or secretion, absorption, and excretion as simple cases of mechanical processes of filtration, osmosis, and diffusion, completely break down when tested by accurate experimental investigation. "To any physiologist," he continues, "who candidly reviews the progress of the last fifty years it must be perfectly evident that, so far from having advanced towards a physico-chemical explanation of life, we are in appearance very much further from one than we were fifty years ago." Thus he disposes of the first reason cited in favour of the rejection of vitalism in biology, viz., that there has been steady progress in the direction of explaining life in terms of physics and chemistry. The second objection to the vitalist position, viz., that it is without meaning as a positive hypothesis, is next passed in review. "This argument in its widest sense," he says, "is undoubtedly based on the metaphysical assumption that the

universe, interpreted as it is in the physical sciences as a universe of matter and energy, corresponds to absolute reality, and is for this reason incapable of any further interpretation. The work of modern philosophy since Berkeley and Hume has shown that the assumption in question is without foundation." (I need hardly reiterate that with regard to this assumption I am in entire agreement with Dr. Haldane's attitude.)

But "the form in which the objection in question really presents itself to most physiologists is that, apart from all metaphysical arguments, vitalism presents no positive working hypothesis capable of being used to advance physiology."

It is with Dr. Haldane's treatment of this aspect of the problem of vitalism versus mechanism,—occupying the latter two thirds of his article,—that I shall more particularly concern myself with at this time.

I shall not dispute the proposition that, in the progress of the science of physiology, physico-chemical theories of living process have broken down all along the line. I readily admit that such theories have in every direction failed to accomplish that mechanical analysis of function which seemed to the physiologists of the later decades of the century to be so nearly within their grasp. Yet it would be grossly inaccurate to assert that the attempt to explain life as mechanism has resulted in nothing but failure. The fact is that mechanism after mechanism has been displayed, through the operation of whose chemical and physical properties the functional activity of the organism is subserved.

On the other hand it is true that the residual phenomena unexplained by these mechanisms may in a sense be held to embody the very essence of the mystery of organisation. It is not difficult to see that in the nature of the case this must be so. It is the penalty of the abstract character of the causal principle employed as the instrument of research. The forging of links in an endless chain of mechanical causation is a never-ending process,—the mystery ever recedes as we pursue it further into the recesses of organisation.

Does the recognition of even such a radical imperfection at the root of the physico-chemical conception really involve its rejection as the characteristic conception of scientific procedure? I do not think that this can be admitted. The objection would hardly be pressed by anyone with regard to the use of the idea in physics and chemistry, although, in the last resort, the criticism of the abstract idea of causality as a final principle, is as valid in that sphere as elsewhere. And if in a more obvious and pre-eminent way the mechanical hypothesis breaks down when it is offered as an explanation of vital phenomena, it does not do so without giving us splendid proof of its capabilities as a working hypothesis. The search for causes has resulted in the revelation of mechanism upon mechanism in the way of structural organisation; process in multicellular organisms is realised through material parts or organs more and more minute, as far as our means of observation enable us to proceed. Must we halt for ever upon the threshold of intracellular organisation? What is there in that organisation that we should feel obliged there to discard the conception of mechanism, elsewhere so serviceable? Do we here enter a new world for the first time? Assuredly not. The real obstacle to a mechanical theory of life is not met with at one point more than another, but all along the line. "Vitalistic or teleological interpretation," it was urged in last year's address, "is not a method which comes to our rescue when a physical interpretation fails us. In so far as it is valid at all, it is one which is present with us and which urges itself upon us *at every stage*, forbidding us ever to mistake a possible mechanical inter-connection of the phenomena of life for the real ground in thought of purposive adaptation."

In referring to the shortcomings of the attempted physico-chemical analysis of living process, Dr. Haldane avers that "we are now far more definitely aware of the obstacles to any advance in this (physico-chemical) direction, and there is not the slightest indication that they will be removed, but rather that, with further increase of knowledge, and more refined methods of physical and chemical investigation, they will only appear more and more difficult to surmount."

So far as I can see there are no more "obstacles" than there ever were to a mechanical view of living process. There must always be possible a "mechanical explanation" of the phenomena so long as observation continues to reveal underlying mechanical arrangements. And even Dr. Haldane does not suggest that we have run up against a blank wall in the experimental investigation of organism according to physical and chemical principles. He does not doubt that "by the further application of these principles we shall continue to extend our knowledge." And in the following extract from a private letter of earlier date than the article under consideration, he expressly disclaims the disposition to set bounds to progress in the direction indicated. "I do not mean," he says here, "that physico-chemical investigations will in any way cease to make as much progress as before in the domain of life, for one can see no limit to the progress of, say, physiological physics or chemistry. Nevertheless, every year makes it clearer that with all this progress we seem to get further and further from physico-chemical explanations of any of the elementary phenomena of biology, growth, development, nutrition, secretion, heredity, excitability, &c."

But when it is conceded that we do actually "make progress in the domain of life" by means of physico-chemical investigation, one is constrained to ask "does not the knowledge so gained, *just so far as it goes*, amount to an actual and genuine scientific explanation of the phenomena concerned"?

It seems to me radically wrong to assume, as Haldane appears to me to do when he speaks of "getting further and further from physico-chemical explanations of the elementary problems of biology," that such an explanation, or, indeed, any explanation of phenomena whatever, is to be conceived merely as an end-product of thought, or a terminal goal of scientific investigation. The explanation and interpretation of vital phenomena is always going on. *Solvitur ambulando.* As we learn the physics and chemistry of "living protoplasm," of those parts and substances which all will admit to be in some sense the embodiment of function, as we determine causes and effects of events in the way of process, and

distinguish the actual from the apparent, and true modes of relatedness from false ; as we thus proceed, I hold that we are, *de facto*, *explaining* living process in terms of mechanism, even if in so doing we may not be saying the last word about its significance.

Dr. Haldane does himself admit that "perfectly satisfactory physical explanations can, for instance, be given of the manner in which contractions of the muscles and of the heart respectively bring about the movements of the limbs and the circulation of the blood." Again, "we can explain, on purely physical and chemical principles, many isolated processes occurring in the living body." But it is pointed out, with perfect justice, that, underlying these more obvious mechanisms, there lies the more subtle operation of a cellular activity which does not yield to a physico-chemical analysis. According to the vitalistic view, any function, or any aspect of function, which is capable of being thus analysed is non-vital. "If we look, however, at the phenomena which are capable of being stated or explained in physico-chemical terms, we see at once that there is nothing in them characteristic of life."

This is, in truth, a short and easy mode of disposing of the mechanical interpretation of function ; but if it be true that the progress of physiology has largely consisted in the elucidation of function-complexes by the recognition of elementary cell-phenomena underlying the grosser mechanical aspect of the processes, then we should appear to be justified in concluding from this reasoning that it is only in the elementary physiological activity of intracellular function that we can recognise any genuine manifestation of vitality.

I see nothing to be gained by the attempt to classify the functions of an organism into those which are characteristic of life and those which are not. Surely any and every process carried on as a part of the life of an organism is characteristic of life, whether it seem to be analysable into physico-chemical process or not.

Nor will it do to admit that explanations in terms of mechanism are appropriate for certain of the operations of organism, and

then to pull up short at the problems involved in intracellular activity and deny the applicability of physico-chemical explanation to the phenomena there manifested. It is admitted on all hands that "the elementary problems of biology,—growth, development, nutrition, secretion, heredity, excitability, &c."—are at bottom intracellular problems. And in my humble opinion, if we knew as many facts regarding the material organisation of living cells, and were able to make the same kind of observation and experiment upon them as we can upon cell-complexes, we should then find that physics and chemistry could do for us exactly the same kind of thing,—not less,—and as certainly not more,—than they have done in explanation of those processes of which, Dr. Haldane thinks, we have already "perfectly satisfactory explanations."

In this connection it may be useful to recall the views upon the same subject of another distinguished young physiologist, as expressed in the interesting address on "the relations between morphology and physiology," to which probably most of you had the pleasure of listening at the opening of the biological section of the Australasian Association at its meeting in Sydney last year. There Professor Martin discourses, among other matters, concerning the limitations of the physiological physico-chemical movement of the last half century, "so far as a complete understanding of life is concerned." He remarks that, "The physiologists, too, having studied the chemistry and physics of phenomena associated with the life of higher animals, have tracked physiological activity into the cell. Here, for the time being, a view of the mechanism is lost, and cellular physiology does not appear capable of being successfully attacked along the same lines of mechanical interpretation which have proved so successful in dealing with the functions of compound organs."

"One must not imagine," he continues, "that morphological or physiological inquiry of the character which has been so fruitfully prosecuted during the last half-century is in any sense exhausted."

The body of the address is occupied in discussing the directions of progress of both morphology and physiology, and emphasis is laid on the fact that, in the case of both disciplines, the essential problems have been followed up to the threshold of the living cell. Thus Dr. Martin proceeds—"For the past fifty years the physiologists have been principally concerned with the analysis of the function of organs as such, and have more or less left aside the physiology of cells. In my opinion they have been quite wise in so doing. In this way all those physiological phenomena which can be measured according to physical standards and interpreted in terms of physics and chemistry and physics have, to a large extent, been separated off from those that cannot. Processes in which cells participate collectively as membranes or organs have been more or less sharply defined from those in which they operate by means of their individuality, and in which cases the phenomena are intracellular. Surely it was wise to ascertain to what extent a physiological result was due to the physical or chemical properties of the matter concerned, in order to know at what point the intervention of cellular activities is necessary" Throughout the whole discussion of the various phases of the physiological problem dealt with, Professor Martin appears to agree with Dr. Haldane that in every case of function-analysis the most characteristic and essential quality of the process has been "tracked" into the cell. But if I rightly interpret Dr. Martin's attitude, it differs from Dr. Haldane's in that the former finds no necessity for the abandonment, but only for the further prosecution of the methods of the last fifty years. As a physiologist, he has evidently "no desire to cry a halt at this point," even if "the *known* laws of chemistry and physics seem so hopelessly incapable of furnishing any interpretation" of the problems at issue. It is interesting to compare the attitude taken up by Haldane and Martin respectively in reference to such an apparently established physiological fact as that the tension of oxygen in arterial blood is frequently higher than it is in the air of the lung alveoli. This is interpreted by Haldane as signifying that here we have evidence of a defiance of physico-chemical law,

i.e., that the physical laws of the diffusion of gases do not hold in this case. There is a noteworthy difference in the view of the same facts of gaseous respiratory exchange taken by Martin. In his view the results of experiments show that "the exchange of gas between the blood plasma and the alveolar air is regulated to some extent according to the law of partial pressures." And, with regard to the above-mentioned strikingly anomalous behaviour of the respiratory oxygen (first announced by Bohr, and since confirmed by the work of Haldane and Lorrain Smith), Martin cautiously remarks that this fact "cannot be explained by diffusion across a membrane, with which one is so far acquainted in physical experiments." But it is to be observed that this inadequacy of physical explanation suggests to him, not a break in the continuity of mechanical theory as applicable to the phenomena under consideration, but simply a new physical hypothesis as to the material structure of a membrane which should allow of such novel behaviour. And so on, wherever the known laws of physics and chemistry seem incapable of accounting for the activities manifested in living matter, the question for Martin seems ever to be "if this be not a case of the operation of known mechanism, what is the actual and genuine mechanism underlying it; if the originally supposed mechanism is not the true cause of the operation, what is the real and actual antecedent cause?" And to me this appears to be the only genuinely *scientific* question, the only kind of question answerable by means of experimental scientific procedure.

The question may be brought into relief by the use of a familiar quotation from Clerk Maxwell—"Now one material system can differ from another only in the configuration and motion which it has at a given instant. To explain differences of function and development of a germ without assuming difference of structure is, therefore, to admit that the properties of a germ are not those of a purely material system." Here, of course, it is the one physiological process of development which is in view. For the present purpose we might write "living cell" in place of "germ" or germ-cell. On the lines of Clerk Maxwell's formula, the

general contention here supported might be summarised thus :— Living activity can only be known to scientific investigation as manifested in changes in configuration and motion of certain bodies in space and time. Such a body is for science, therefore, a material system, and, as such, its function or change of motion or configuration implies material structural constitution, *i.e.*, a mechanism embodying and determining the functional change. A dissociation of function and structure—a divorce between mechanism and motion, living or other—is an impossibility for scientific thought.

That another interpretation of organism transcending that of mechanism, is not only possible but necessary for the human intelligence, I have freely admitted. For such a view it may be necessary to hold that as regards its organisation an organism is no mere object in space; in other words, that it is not "a purely material system." Nevertheless, it is only as an object in space that it can become for us an object of scientific investigation—as part of a material system exhibiting configuration and motion. It is with the changes in motion and configuration manifested by living objects in space that biology, both on its morphological and physiological sides, as a scientific discipline, has to do. And if, as I firmly believe, the conception of organism as a material system is inadequate to express the full concrete reality which organisation possesses for thought; this imperfection is to be remedied, not by the intercalation of the teleological conception at a supposed break in the continuity of possible mechanical interpretation—a break which represents merely the present limit of structural observation—but by a complete philosophical re-interpretation—a philosophical reconstruction—of biological fact, in the light of its significance for the general theory of knowledge.

I feel sure that Dr. Haldane would emphatically demur to my describing his proposition as one which aims at the intrusion of one category of explanation into the sphere of operation of a radically different one. But his assertion of a failure on the part of the mechanical principle to explain the elementary phenomena

of organisation in the same manner in which it is admitted to explain certain non-elementary "processes occurring in the living body," and his demand at that point for the operation of another principle of explanation is, to my mind, tantamount to such an intrusion.

It may, however, be useful to endeavour to ascertain from other statements what precisely it is that Dr. Haldane thinks the new vitalism may do for biology. In the letter from which I have already quoted, the writer says: "It seems to me that we do want new working hypotheses for co-ordinating observations as to these elementary phenomena, and that just as the conceptions of mass and energy differentiated physics from mathematics, so the new biological conceptions will differentiate biology from the physical sciences. When this time comes we shall have got out of the present rather barren controversies between vitalists and anti-vitalists. These controversies will die of inanition, just like the old controversies about the possibility of an absolute vacuum, which used to perplex the physicists and mathematicians. There will then be a distinctively biological way of looking at organisms and their environment, just as there is a distinctively physical and a distinctively mathematical way of looking at the world."

What the precise character of these new and distinctively biological conceptions is to be, beyond the fact that they must be vitalistic, purposive, or teleological, I find it rather difficult to determine, although the latter and major portion of the Nineteenth Century article is devoted to the vindication and defence of vitalism as a positive working hypothesis.

In the attempt to demonstrate the positive content of this hypothesis and its alleged contribution to the advancement of physiological science, the writer summarises the change in the modern point of view, in relation to three typical series of functional facts, viz., those of cell-growth and maintenance; of glandular secretion and absorption; and of respiration.

As regards the first of these, it is pointed out that "the deposit of new material during growth only occurs in immediate

association with a multitude of other processes, which we may distinguish as absorptive, excretory, respiratory, metabolic, &c., and which, occurring as they do in such unison that the cell develops and maintains itself, are characteristic of life." This he justly points out to be a great advance on the idea that organic growth was to be regarded as essentially similar to a process of crystallisation.

As regards the secretion and absorption of material by the glands and intestine, it is shown that these cannot any longer be regarded as due simply to filtration and diffusion, in that (1) the secreting or absorbing surface is always composed of living cells; (2) that the occurrence of secretion and apparently also of absorption involves processes of building up or growth, and breaking down or waste of the cell substance, and is bound up with various changes—respiratory, metabolic, electrical, &c.,—which occur in such unison that the secreting surface maintains itself; (3) that these processes are similar to those occurring in other cells.

Again, in regard to respiration he points out (1) that oxidation occurs within living cells; (2) that its occurrence is intimately associated with the various other characteristic evidences of vital activity occurring in equally characteristic unison; (3) that it occurs in all the cells of the body. And, he continues—"These results not only imply the failure of particular theories of growth, secretion, respiration, heat production, &c., but they entirely bear out the vitalistic contention that the life of an organism in its characteristic aspects can only be studied and understood as a whole, and that attempts to analyse life into a mere series of physical and chemical processes are based on a mistaken theory." "It is evident from the illustrations just given that the physiological comparison of cell with cell, or organism with organism, has led to an enormously increased insight into life, so that in this respect also the vitalistic theory has turned out to be an excellent working hypothesis. But for misleading physico-chemical theories the very fruitful method of comparing with one another different forms of vital activity might have been adopted

all along, and would evidently have led to far more steady and continuous advance."

The writer then contrasts organism with mechanism with reference to the phenomena of self-repair, and adaptation to change in environment, of which he remarks that physico-chemical physiology has failed to give any account, although they may be traced in every elementary physiological process. And positive harm is done when the attention is directed away from these characteristic features of organisation, instead of towards these, as the assumption of a vital principle did.

On these grounds it is alleged that vitalism embodied not only a negative but a positive working hypothesis of great value.

Now I quite fail to see, in the considerations stated, any sufficient reason for refusing to persevere in the mode of explanation by which admittedly we have been led up to the present problems of cell-physiology. Nor can I see wherein, in regard to the phenomena mentioned, vitalism has operated as a working hypothesis distinct from those principles of physics and chemistry which we elsewhere invoke in explanation of changes of the motion and configuration of a material system.

The facts of development, growth, maintenance, adaptation and self-repair, to which Dr. Haldane alludes, are facts which are as patent to the physico-chemical investigator of life as to the vitalist. He has no desire to blink their occurrence. For him, also, the nature of the processes having those particular aspects, forms part of the subject matter of scientific research. It cannot be admitted that there is a single feature of the three lines of discovery adduced as instances of the operation of "vitalism" as a working hypothesis, which is in its nature beyond the recognition of science, working to explain phenomena from the physico-chemical point of view. As a matter of fact the present aspect which each of these problems presents is the fruit, not of vitalistic hypothesis, but of a triumphant reduction of all the grosser aspects of living process as cases of the operation of ordinary mechanical principles. It is not the reproach, but the reward, of modern physical biology that the result of its brilliant analysis is that the essential

problems of life appear now to await solution in the arena of intra-cellular structure and function.

The older vitalists demanded a recognition of the non-mechanical character of the grosser aspects of living process. Even by the neo-vitalists it is now admitted that at least many of these can be explained "on purely physical and chemical principles," but they now turn round and calmly tell us that processes so explainable are not "characteristic of life." Yet it is solely the advances in the physico-chemical analysis of grosser function and structure which have enabled us to re-state the problems involved in the newer and more elementary terms of intracellular process. And with regard to the facts involved in the processes above referred to, of development, growth and maintenance, adaptation, and self-repair, it seems quite unwarrantable to predict that physico-chemical analysis will prove more futile here than at any previous stage of scientific development.

It is true, for instance, that the processes of secretion and absorption of material by cells can no longer be conceived as due simply to diffusion and filtration, but all that necessarily follows from the admission is the concession that the processes are in reality more complicated than was formerly supposed. Apparently there is involved an actual selection of material on the part of the cells concerned. Is such behaviour after all entirely outside the scope of all possible physico-chemical explanation, as the vitalist alleges it to be? Verworn remarks upon this very point that "The principle upon which this phenomenon is based is evidently the same as that which controls in general atoms and molecules, namely, affinity. It is surely no less wonderful that an atom of phosphorus unites very easily with an atom of oxygen, but not with an atom of platinum, than that an intestinal epithelium-cell takes up fat-droplets, but never pigment-granules. And it is no less comprehensible that a Vampyrella surrounds with its body-protoplasm and digests only Spirogyra threads and no other bodies, than that a drop of rancid oil, as Gad has shown, sends out amoeboid processes to an alkaline liquid, and uses the alkali

for the manufacture of soap, but is inactive toward an acid liquid."

Again, one may ask, is not the repair of mutilated crystals a phenomenon which is worthy of being placed alongside the no doubt far more complicated phenomena connected with the self-repair of organisms? The regeneration of the other half of a hemi-gastrula resulting from the destruction *in situ* of one of the first two blastomeres of a developing ovum, however determined, must involve most highly complicated material rearrangements, and the process in the present state of knowledge must be admitted to be practically unintelligible as a mechanical procedure. But can one say so very much more with respect to the regeneration of the ideal form of a crystal which has undergone mutilation?

No one, I take it, would submit these parallels as of equal degrees of complexity. Yet, though the phenomena concerned may be widely incommensurate, as purely objective phenomena they suggest somewhat analogous explanations.

In the latter portion of his paper Dr. Haldane seeks to point out the "way out of the difficulty in which the shortcomings of both the physico-chemical and vitalistic theories have placed physiology."

This attempt he makes with the aid of an appeal to the modern development of scientific anatomy or morphology.

"The fundamental assumption of morphology is," he says, "that each part of an organism is determined as regards its mode of existence by its relations to other parts. That this determination is real and not merely apparent, is shown by the facts (1) that morphological plan is so persistent in spite of disturbing influences; (2) that parts which are removed tend to be reproduced." It is this conception of a morphological plan which is regarded as the vivifying principle of modern anatomy. In other words, it is the idea of homology as morphological identity. I think that upon the whole it is correct to say that it is this idea which is specially characteristic of the morphology of the latter half of the century. But it seems to me that the real ground of the principle as operative in modern science is entirely

misconceived, when it is stated as follows :—"The ground idea of the new anatomy was evidently that of the existence of an immanent type or plan which an organism or group of allied organisms adheres to through every variety of outward modification. This idea dominates morphology and differentiates it from other sciences, just as the ideas of matter and energy dominate and differentiate physics." Such a statement of morphological faith might indeed have emanated from such a scientific anatomist as Sir Richard Owen, but it will certainly not symbolise the practically unanimous views of more recent morphologists. For them, "immanent type or plan" undoubtedly resolves itself into a community of structural character due to actual blood-relationship; an ideal "adherence to morphological plan" is reducible simply to community of origin.

This view of the essential nature of "homology" will alone afford a rational explanation of detailed morphological relationships. According to Dr. Haldane the conception of each part of an organism, regarded morphologically, "evidently involves the conception of its morphological relationships to other parts." In other words, the conception of each part involves that of the whole. "We can mentally separate the parts of a physical structure from the other parts of the same structure, but we cannot do so with the parts of a morphological structure." But whenever we seek to translate into detail what the actual morphological relationships of parts signify, *i.e.*, from the strictly morphological point of view, and apart from their functional significance, we find that we interpret these relationships systematically from the point of view of a theory of descent, and not from that of the existence of "an immanent type or plan" to which the organism "adheres through every variety of outward modification." No doubt "the method of comparing different organisms and different stages in the development of the same organism enables the morphologist to perceive a definite correlation among the parts," but the guiding hypothesis with which he is armed when endeavouring to read unity into the diversity of structural modification—to discover true morphological

identity underlying manifold differences—is undoubtedly that of relationship by common descent, and no mere ideal of unity of type. And this is the case quite independently of the question whether or not the "modern doctrines of relationship by descent, heredity, and gradual differentiation of species by natural selection have furnished a key to" a physico-chemical interpretation of life. On the vitalistic view, of course these doctrines must be held to represent no advance along the line of such interpretation. Dr. Haldane holds that "the doctrine of natural selection does not in any way offer a physico-chemical explanation of the means by which the morphological and physiological characters of an organism are modified." Now this is just what it appears to me natural selection does offer, *so far as it goes*. It is an attempt to explain the facts of the admitted evolution of organic forms as a series of events linked together by purely causal connection. Last year I insisted upon its inadequacy as a complete principle of explanation on account of its fundamental assumption of (unexplained) variability. But that it is, nevertheless, an actually operative factor in development, through whose use we may be said to make progress in the recognition of the causal sequences in biological phenomena, I can see no reason for doubting. Yet no more here than anywhere else are we exempt from the inevitable re-interpretation of all such phenomena, when the causal principle is assigned its rightful place in a true theory of knowledge as an abstract and incomplete principle of interpretation.

It is also true that for a complete analysis of the facts of morphology we urgently require a tenable theory of heredity. And it is objected that "no attempt worthy of serious consideration has ever been made to furnish even the outlines of a physico-chemical theory of heredity."

The question of heredity is obviously bound up with that of the structure and properties of the living matter which is carried over from parent to offspring. Everyone must admit that the substance of the oosperm is in some sense the embodiment and the carrier of the characteristics of the parent organisms. As a

material system the germ must necessarily possess, registered either in its physical structure, or in its chemical composition, or in both together, potential equivalents of those properties in which it resembles the parental organisation. To deny the existence of some such physico-chemical embodiment seems to me tantamount to asserting, not only that the properties of a germ are not those of a purely material sytem, but that the entire phenomena of reproduction are essentially unintelligible.

To admit so much is of course a very different thing from admitting the whole contention of the thorough-going preformationists. It amounts to no more than the assertion of a structural basis for organisation, not only in the ovum but in the developing organism itself. "Continuity of organisation," says Whitman, "does not of course mean preformed organs, it means only that a definite structural foundation must be taken as the starting-point of each organism," whose "organic unity must depend on intrinsic properties no less than does molecular unity."

"The indubitable fact on which we now build is no bit of inorganic homogeneity, but the ready-formed, living germ, with an organisation cut directly from a pre-existing, parental organisation of the same kind. The essential thing is not simply continuity of germ-substance of the same chemico-physical constitution, but actual identity of germ-organisation with stirp-organisation."

The facts of regeneration are confidently appealed to in order to support the contention that the differentiation of structure in an organism is governed by a general morphological idea of organic unity, and not by any sort of mechanical predetermination of its structural parts. And one may frankly admit the entire inability of conceiving how, by some physical arrangement of determinants, the half-embryo which results from the development *in situ* of one only of the first two blastomeres, should possess the capacity of regenerating the other half. Yet we are not entitled to adopt the extreme views formerly expressed by Driesch, which assume the absolute isodynamy of the early embryonic cells, according to which theory they may be "thrown about at will, like balls in a

pile, without the least impairment of their power of development." "Their prospective value," according to Driesch, "is a function, of their position in the whole," which, in this connection, means their morphological relations to each other. Yet it has been shown in a large number of cases at a very early stage of development, and in some cases even from the first, there exists a degree of qualitative differentiation of the germ-material. Such was shown to be present in Amphioxus and Nereis by E. B. Wilson, several years ago. And if the fact of the regeneration of the missing halves of hemi-embryos proved fatal to the mosaic theory of development in its original form, more recent observations have shown that a fairly extensive predetermination of cytoplasmic regions may in certain cases be shown to exist. Thus in the case of the egg of Beroe, the experiments of Driesch and Morgan, and more lately those of Fischel, have shown that an isolated blastomere of the two- or four-celled stage gives rise to a half- or quarter-embryo; and also if part of an unsegmented egg were removed the rest generated an incomplete larva, showing certain defects which represent the portions removed.

A conclusive evidence of underlying mechanical arrangements in germinal structure would seem, moreover, to be derivable from experiments upon the influence of gravity upon the development of frogs' ova. In 1894, O. Schultze discovered that if the egg of a frog be turned upside down when in the two-cell stage, a whole embryo, (or half of a double embryo) might arise from each blastomere instead of a half-embryo, as in the normal development, and that the axes of these embryos show no constant relation to one another. Again, if, after destruction of one blastomere, the other be allowed to remain in its normal position, a half-embryo always results, precisely as described by Roux. If, on the other hand, the blastomere be inverted it may give rise either to a half-embryo or to a whole dwarf. According to Wilson, from whom I have largely quoted in reference to these experiments, we have here the most conclusive evidence that each of the two blastomeres contains all the materials, nuclear and cytoplasmic, necessary for the formation of a whole body; and that these materials

may be used to build a whole body or half-body, according to the grouping they assume. After the first cleavage takes place, each blastomere is *set*, as it were, for a half-development, but not so firmly that a re-arrangement is excluded. It is through the interpretation of facts of this kind that Wilson believes that we can "reconcile the theories of cytoplasmic localisation and mosaic development with the hypothesis of cytoplasmic isotropy. Primarily the egg-cytoplasm is isotropic in the sense that its various regions stand in no fixed and necessary relation with the parts to which they respectively give rise. Secondarily, however, it may undergo differentiations through which it acquires a definite regional predetermination, which becomes ever more firmly established as development advances. This process does not, however, begin at the same time, or proceed at the same rate in all eggs. Hence the eggs of different animals may vary widely in this regard, at the time cleavage begins, and hence may differ as widely in their power of response to changed conditions."

For our present purpose the importance of the facts quoted lies in their testimony to the general fact of an ultra-microscopical organised structure of germ cells, which embodies and subserves the intracellular expressions of living activity, just in the same way as the visible bodily organs embody the more obvious and familiar aspects of bodily function.

It must therefore be maintained that neither the obscurity of the problem of heredity, nor the leadings of the extraordinarily striking phenomena of regeneration can be regarded as absolutely incapable of being brought into line with other biological facts as causally determined in the mechanical sense, far as we are at present from any such achievement.

And in the present connection it cannot be admitted that we are under any sort of compulsion to abandon the natural-historical interpretation of homology—the true guiding hypothesis of modern morphology—simply because we cannot effect a definitive analysis of its more important factors. Inability to do this does not, for example, deprive me of the solid conviction that the morphological relationship existing between, say, the presence of a

marsupial pouch and an inflected mandibular angle, is to be interpreted simply as a common family character, transmitted by descent, and deriving its whole meaning from the fact of this transmission ; and not as an instance of any recondite conformity to an immanent ideal "type."

Thus the supposed parallel or contrast between the progress of morphology and physiology will not help the vitalist argument. For in reality, morphology, just as much as physiology, has been advancing by the aid of hypotheses which are conceived as every bit as mechanical as those which have achieved no small measure of success in physiological science.

In neither case can we afford to dispense with that category of explanation which alone is appropriate to the investigation of the operation of any material system, extended in space, and manifesting its phenomena as a series of events in time.

A final quotation from the article under criticism will suffice to summarise the question at issue.

"All that is really shown by the partial success which has attended the application of physical and chemical principles of explanation in physiology is that in the course of investigation it is often possible to ignore for the time the distinctive features of life. For certain scientific purposes we may treat some part of the body as a mechanism, without taking into consideration the manner in which it is controlled and maintained; and in this way results of great value have been attained. But in doing all this we are deliberately ignoring or abstracting from all that is characteristic of life in the phenomena dealt with. The action of each bodily mechanism, the composition and structure of each organ, the intake and output of energy from the body, are all mutually determined and connected with one another in such a way as at once to distinguish a living organism from anything else. As this mutual determination is the characteristic mark of what is living, it cannot be ignored in the framing of fundamental working hypotheses."

With nearly the whole of this statement I am in substantial agreement. For, "certain scientific purposes," I should put, "for

all strictly scientific purposes"; and as regards the last sentence, I consider that the teleological determination there referred to is incapable of incorporation in the working hypotheses of experimental science, except to that extent to which it can be translated into terms appropriate to the connections and relations of a material system.

But, after all, the points of agreement far outweigh the points of difference in the two stand-points compared. For both alike, the interpretation of the phenomena of life as in their essential character merely mechanical is based upon the untenable "metaphysical assumption that the universe, interpreted as it is in the physical sciences as a universe of matter and energy, corresponds to absolute reality, and is for that reason incapable of any further interpretation."

The full significance of Nature is not to be apprehended by the externalising operation of purely scientific interpretation, be the scope of its investigations never so extended. Not even a complete "astronomical knowledge" of the molecular dance of elementary physical particles could absolve us from the necessity of finding the ultimate explanation of all phenomena in terms of that single spiritual principle which alone makes knowledge possible, and for which alone even material bodies either live or move or have any being at all.

"Ihr folget falscher Spur;
Denkt nicht, wir scherzen!
Ist nicht der Kern der Natur
Menschen im Herzen?"

On the motion of Mr. A. H. S. Lucas, M.A., a very cordial vote of thanks was accorded to the President for his interesting Address.

The Hon. Treasurer, Mr. P. N. Trebeck, presented the balance sheet, duly certified by the Auditors. The Society's total income for the financial year ending December 31st, on both General and Bacteriological Accounts was £2,296 10s. 2d.; the total